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DARWIN'S THEORY OF MAN'S DESCENT AS IT STANDS TO-DAY[1]

MY LORD MAYOR, MR. VICE-CHANCELLOR, LADIES AND GENTLEMEN,

My first duty as your president, and it is a very pleasant one, is to send the following message in your name to H.R.H. The Prince of Wales:

YOUR ROYAL HIGHNESS,

The British Association for the Advancement of Science, now assembled in Leeds to begin another session, can not allow your year of office to terminate without offering to you sincere and humble congratulations on the happy results which have attended your presidency. A year ago, in the historic city of Oxford, you did British science the signal honor of coming among us as our president; the meeting you then inaugurated set a standard which future gatherings will strive to emulate. The inspiring message you then addressed to us, and through us to men of science in every part of the empire, has already borne fruit. We are within sight of a closer union, for which the association itself has always striven, between men of science overseas and their colleagues at home, in their endeavor to solve problems of imperial concern. It is too soon as yet to assess the value of the harvest of science planted under your ægis, for the best vintages of science mature slowly, but of this we are certain: the interest Your Royal Highness has taken in the work of this association will prove a permanent source of encouragement for all who work for the betterment of life through increase of knowledge. To-night we proudly add your presidential banner to those of the great men of science who have presided over this association since its inception at York ninety-six years ago.

In olden times men kept their calendars by naming each year according to its outstanding event. I have no doubt that in future times the historian of this association, when he comes to distinguish the presidential year which opened so auspiciously in Oxford twelve months ago, will be moved to revert to this ancient custom and name it the Prince's Year. And I am under no misapprehension as to what will happen when our historian comes to the term which I have now the honor of inaugurating at Leeds; he will immediately relapse to the normal system of numerical notation. Nor will our historian fail to note, should he be moved to contrast the meeting at Oxford with

[1] The presidential address before the British Association for the Advancement of Science given at Leeds on August 31.

that which now begins at Leeds, that some mischievous sprite seems to have tampered with the affairs of this association. For how otherwise could he explain the fortune which fell to ancient Oxford, the home of history? To her lot fell a brilliant discourse on the application of science to the betterment of human lives, while Leeds, a city whose life's blood depends on the successful application of science to industry, had to endure, as best she could, a discourse on a theme of ancient history. For the subject of my address is man's remote history. Fifty-five years have come and gone since Charles Darwin wrote a history of man's descent. How does his work stand the test of time? This is the question I propose to discuss with you to-night in the brief hour at my disposal.

In tracing the course of events which led up to our present conception of man's origin, no place could serve as a historical starting-point so well as Leeds. In this city was fired the first verbal shot of that long and bitter strife which ended in the overthrow of those who defended the Biblical account of man's creation and in a victory for Darwin. On September 24, 1858—sixty-nine years ago—the British Association assembled in this city just as we do to-night; Sir Richard Owen, the first anatomist of his age, stood where I now stand. He had prepared a long address, four times the length of the one I propose to read, and surveyed, as he was well qualified to do, the whole realm of science; but only those parts which concern man's origin require our attention now. He cited evidence which suggested a much earlier date for the appearance of man on earth than was sanctioned by Biblical records, but poured scorn on the idea that man was merely a transmuted ape. He declared to the assembled association that the differences between man and ape were so great that it was necessary, in his opinion, to assign mankind to an altogether separate order in the animal kingdom. As this statement fell from the president's lips there was at least one man in the audience whose spirit of opposition was roused—Thomas Henry Huxley—Owen's young and rising antagonist.

I have picked out Huxley from the audience because it is necessary, for the development of my theme, that we should give him our attention for a moment. We know what Huxley's feelings were towards Owen at the date of the Leeds meeting. Six months before, he had told his sister that "an internecine feud rages between Owen and myself," and on the eve of his departure for Leeds he wrote to Hooker: "The interesting question arises: shall I have a row with the great O. there?" I am glad to say the Leeds meeting passed off amicably, but it settled in Huxley's mind what the "row" was to be about when it came. It was to concern man's rightful position in the scale of living things.

Two years later, in 1860, when this association met in Oxford, Owen gave Huxley the opportunity he desired. In the course of a discussion Owen repeated the statement made at Leeds as to man's separate position, claiming that the human brain had certain structural features never seen in the brain of anthropoid apes. Huxley's reply was a brief and emphatic denial with a promise to produce evidence in due course—which was faithfully kept. This opening passage at arms between our protagonists was followed two days later by that spectacular fight—the most memorable in the history of our association—in which the Bishop of Oxford, the representative of Owen and of orthodoxy, left his scalp in Huxley's hands. To make his victory decisive and abiding, Huxley published, early in 1863, "The Evidences of Man's Place in Nature," a book which has a very direct bearing on the subject of my discourse. It settled for all time that man's rightful position is among the primates, and that, as we anatomists weigh evidence, his nearest living kin are the anthropoid apes.

My aim is to make clear to you the foundations on which rest our present-day conception of man's origin. The address delivered by my predecessor from this chair at the Leeds meeting of 1858 has given me the opportunity of placing Huxley's fundamental conception of man's nature in a historical setting. I must now turn to another issue which Sir Richard Owen merely touched upon but which is of supreme interest to us now. He spent the summer in London, just as I have done, writing his address for Leeds and keeping an eye on what was happening at scientific meetings. In his case something really interesting happened. Sir Charles Lyell and Sir Joseph Hooker left with the Linnean Society what appeared to be an ordinary roll of manuscript, but what in reality was a parcel charged with high explosives, prepared by two very innocent-looking men—Alfred Russel Wallace and Charles Darwin. As a matter of honesty it must be admitted that these two men were well aware of the deadly nature of its contents, and knew that if an explosion occurred, man himself, the crown of creation, could not escape its destructive effects. Owen examined the contents of the parcel and came to the conclusion that they were not dangerous; at least, he manifested no sign of alarm in his presidential address. He dismissed both Wallace and Darwin, particularly Darwin, in the briefest of paragraphs, at the same time citing passages from his own work to prove that the conception of natural selection as an evolutionary force was one which he had already recognized.

As I address these words to you I can not help marvelling over the difference between our outlook to-day and that of the audience which Sir Richard Owen had to face in this city sixty-nine years ago. The vast assemblage which confronted him was convinced, al-st without a dissentient, that man had appeared on rth by a special act of creation; whereas the audi-e which I have now the honor of addressing, and t larger congregation which the wonders of wireless ng within the reach of my voice, if not convinced rwinists are yet prepared to believe, when full ofs are forthcoming, that man began his career as umble primate animal, and has reached his present te by the action and reaction of biological forces ch have been and are ever at work within his body d brain.

his transformation of outlook on man's origin is of the marvels of the nineteenth century, and to how it was effected we must turn our attention for ittle while to the village of Down in the Kentish ands and note what Charles Darwin was doing on very day that Sir Richard Owen was delivering address here in Leeds. He sat in his study strug-g with the first chapter of a new book; but no one esaw, Owen least of all, that the publication of the pleted book, *The Origin of Species,* fifteen months r (1859), was to effect a sweeping revolu-n in our way of looking at living things and to tiate a new period in human thought—the Dar-ian period—in which we still are. Without know-it, Darwin was a consummate general. He did not nch his first campaign until he had spent twenty-years in stocking his arsenal with ample stores of ed and assorted fact. Having won territory with *Origin of Species,* he immediately set to work onsolidate his gains by the publication in 1868 of ther book, *The Variation of Animals and Plants er Domestication*—a great and valuable treasury biological observation. Having thus established an anced base, he moved forwards on his final ob-ive—the problem of human beginnings—by the lication of *The Descent of Man* (1871), and that del capitulated to him. To make victory doubly ain he issued in the following year—1872—*The pression of the Emotions in Man and Animals.* ny a soldier of truth had attempted this citadel ore Darwin's day, but they failed because they had ther his generalship nor his artillery.

Will Darwin's victory endure for all time? Before mpting to answer this question, let us look at what d of book *The Descent of Man* is. It is a book history—the history of man, written in a new way e way discovered by Charles Darwin. Permit me llustrate the Darwinian way of writing history. If history of the modern bicycle had to be written in orthodox way, then we should search dated rec-s until every stage was found which linked the -wheeled hobby-horse, bestrode by tall-hatted fash-ble men at the beginning of the nineteenth cen-y, to the modern "jeopardy" which now flashes past us in country lanes. But suppose there were no dated records—only a jumble of antiquated machines stored in the cellar of a museum. We should, in this case, have to adopt Darwin's way of writing history. By an exact and systematic comparison of one machine with another we could infer the relationship of one to another and tell the order of their appearance, but as to the date at which each type appeared and the length of time it remained in fashion, we could say very little. It was by adopting this circumstantial method that Darwin succeeded in writing the history of man. He gathered historical documents from the body and be-havior of man and compared them with observations made on the body and behavior of every animal which showed the least resemblance to man. He studied all that was known in his day of man's em-bryological history and noted resemblances and differ-ences in the corresponding histories of other animals. He took into consideration the manner in which the living tissues of man react to disease, to drugs and to environment; he had to account for the existence of diverse races of mankind. By a logical analysis of his facts Darwin reconstructed and wrote a history of man.

Fifty-six years have come and gone since that his-tory was written; an enormous body of new evidence has poured in upon us. We are now able to fill in many pages which Darwin had perforce to leave blank, and we have found it necessary to alter details in his narrative, but the fundamentals of Darwin's outline of man's history remain unshaken. Nay, so strong has his position become that I am convinced that it never can be shaken.

Why do I say so confidently that Darwin's position has become impregnable? It is because of what has happened since his death in 1882. Since then we have succeeded in tracing man by means of his fossil re-mains and by his stone implements backwards in time to the very beginning of that period of the earth's history to which the name Pleistocene is given. We thus reach a point in history which is distant from us at least 200,000 years, perhaps three times that amount. Nay, we have gone farther, and traced him into the older and longer period which preceded the Pleistocene—the Pliocene. It was in strata laid down by a stream in Java during the latter part of the Plio-cene period that Dr. Eugene Dubois found, ten years after Darwin's death, the fossil remains of that re-markable representative of primitive humanity to which he gave the name *Pithecanthropus,* or ape-man; from Pliocene deposits of East Anglia Mr. Reid Moir has recovered rude stone implements. If Darwin was right, then as we trace man backwards in the scale of time he should become more bestial in form—nearer to the ape. That is what we have found. But if we regard *Pithecanthropus* with his small and simple yet human brain as a fair representative of the men of the Pliocene period, then evolution must have proceeded at an unexpectedly rapid rate to culminate to-day in the higher races of mankind.

The evidence of man's evolution from an ape-like being, obtained from a study of fossil remains, is definite and irrefutable, but the process has been infinitely more complex than was suspected in Darwin's time. Our older and discarded conception of man's transformation was depicted in that well-known diagram which showed a single file of skeletons, the gibbon at one end and man at the other. In our original simplicity we expected, as we traced man backwards in time, that we should encounter a graded series of fossil forms—a series which would carry him in a straight line towards an anthropoid ancestor. We should never have made this initial mistake if we had remembered that the guide to the world of the past is the world of the present. In our time man is represented not by one but by many and diverse races—black, brown, yellow and white; some of these are rapidly expanding, others are as rapidly disappearing. Our searches have shown that in remote times the world was peopled, sparsely it is true, with races showing even a greater diversity than those of to-day, and that already the same process of replacement was at work. To unravel man's pedigree, we have to thread our way, not along the links of a chain, but through the meshes of a complicated network.

We made another mistake. Seeing that in our search for man's ancestry we expected to reach an age when the beings we should have to deal with would be simian rather than human, we ought to have marked the conditions which prevail amongst living anthropoid apes. We ought to have been prepared to find, as we approached a distant point in the geological horizon, that the forms encountered would be as widely different as are the gorilla, chimpanzee and orang, and confined, as these great anthropoids now are, to limited parts of the earth's surface. That is what we are now realizing; as we go backwards in time we discover that mankind becomes broken up, not into separate races as in the world of to-day, but into numerous and separate species. When we go into a still more remote past they become so unlike that we have to regard them not as belonging to separate species but different genera. It is amongst this welter of extinct fossil forms which strew the ancient world that we have to trace the zigzag line of man's descent. Do you wonder we sometimes falter and follow false clues?

We committed a still further blunder when we set out on the search for man's ancestry: indeed, some of us are still making it. We expected that man's evolution would pursue not only an orderly file of stages, but that every part of his body—skull, brain, jaws, teeth, skin, body, arms and legs—would at each stage become a little less ape-like, a little more man-like. Our searches have shown us that man's evolution has not proceeded in this orderly manner. In some extinct races, while one part of the body has moved forwards another part has lagged behind. Let me illustrate this point because it is important. We now know that, as Darwin sat in his study at Down, there lay hidden at Piltdown, in Sussex, not thirty miles distant from him, sealed up in a bed of gravel, a fossil human skull and jaw. In 1912, thirty years after Darwin's death, Mr. Charles Dawson discovered this skull and my friend Sir Arthur Smith Woodward described it, and rightly recognized that skull and jaw were parts of the same individual, and that this individual had lived, as was determined by geological and other evidence, in the opening phase of the Pleistocene period. We may confidently presume that this individual was representative of the people who inhabited England at this remote date. The skull, although deeply mineralized and thick-walled, might well have been the rude forerunner of a modern skull, but the lower jaw was so ape-like that some experts denied that it went with the human fossil skull at all, and supposed it to be the lower jaw of some extinct kind of chimpanzee. This mistake would never have been made if those concerned had studied the comparative anatomy of anthropoid apes. Such a study would have prepared them to meet with the discordances of evolution. The same irregularity in the progression of parts is evident in the anatomy of *Pithecanthropus*, the oldest and most primitive form of humanity so far discovered. The thigh-bone might easily be that of modern man, the skull-cap that of an ape, but the brain within that cap, as we now know, had passed well beyond an anthropoid status. If merely a lower jaw had been found at Piltdown an ancient Englishman would have been wrongly labelled "Higher anthropoid ape"; if only the thigh-bone of *Pithecanthropus* had come to light in Java, then an ancient Javanese, almost deserving the title of anthropoid, would have passed muster as a man.

Such examples illustrate the difficulties and dangers which beset the task of unravelling man's ancestry. There are other difficulties; there still remain great blanks in the geological record of man's evolution. As our search proceeds these blanks will be filled in, but in the meantime let us note their nature and their extent. By the discovery of fossil remains we have followed man backwards to the close of the Pliocene—a period which endured at least for a quarter of a million years, but we have not yet succeeded in tracing him through this period. It is true that we have found fossil teeth in Pliocene deposits which may be those of n ape-like man or of a man-like ape; until we find her parts of their bodies we can not decide. When e pass into the still older Miocene period—one which as certainly twice as long as the Pliocene—we are in e heyday of anthropoid history. Thanks to the bors of Dr. Guy E. Pilgrim, of the Indian Geolog-al Survey, we know already of a dozen different nds of great anthropoids which lived in Himalayan ngles during middle and later Miocene times; we ow of at least three other kinds of great anthropoids hich lived in the contemporary jungles of Europe. nfortunately we have found as yet only the most sistant parts of their bodies—teeth and fragments of w. Do some of these fragments represent a human cestor? We can not decide until a lucky chance ings to light a limb-bone or a piece of skull, but no e can compare the teeth of these Miocene anthro-oids with those of primitive man, as has been done thoroughly by Professor William K. Gregory, and cape the conviction that in the dentitions of the ex-ct anthropoids of the Miocene jungles we have the cestral forms of human teeth.

It is useless to go to strata still older than the Mio-e in search of man's emergence; in such strata we ve found only fossil traces of emerging anthropoids. l the evidence now at our disposal supports the con-sion that man has arisen, as Lamarck and Darwin spected, from an anthropoid ape not higher in the ological scale than a chimpanzee, and that the date which human and anthropoid lines of descent began diverge lies near the beginning of the Miocene riod. On our modest scale of reckoning, that gives n the respectable antiquity of about one million ars.

Our geological search, which I have summarized all briefly, has not produced so far the final and con-sive evidence of man's anthropoid origin; we have t found as yet the human *imago* emerging from its thropoid encasement. Why, then, do modern an-opologists share the conviction that there has been anthropoid stage in our ancestry? They are no re blind than you are to the degree of difference ich separates man and ape in structure, in appear-e and in behavior. I must touch on the sources of s conviction only in a passing manner. Early in present century Professor G. H. F. Nuttall, of mbridge University, discovered a trustworthy and ct method of determining the affinity of one spe-s of animal to another by comparing the reactions their blood. He found that the blood of man and t of the great anthropoid apes gave almost the e reaction. Bacteriologists find that the living an-opoid body possesses almost the same susceptibili-to infections, and manifests the same reactions, as s the body of man. So alike are the brains of man and anthropoid in their structural organization that surgeons and physiologists transfer experimental observations from the one to the other. When the human embryo establishes itself in the womb it throws out structures of a most complex nature to effect a connection with the maternal body. We now know that exactly the same elaborate processes occur in the anthropoid womb and in no other. We find the same vestigial structures—the same "evolutionary post-marks"—in the bodies of man and anthropoid. The anthropoid mother fondles, nurses and suckles her young in the human manner. This is but a tithe of the striking and intimate points in which man resembles the anthropoid ape. In what other way can such a myriad of coincidences be explained except by presuming a common ancestry for both?

The crucial chapters in Darwin's *Descent of Man* are those in which he seeks to give a historical account of the rise of man's brain and of the varied functions which that organ subserves. How do these chapters stand to-day? Darwin was not a professional anatomist and therefore accepted Huxley's statement that there was no structure in the human brain that was not already present in that of the anthropoid. In Huxley's opinion the human brain was but a richly annotated edition of the simpler and older anthropoid book, and this edition, in turn, was but the expanded issue of the still older original primate publication. Since this statement was made thousands of anatomists and physiologists have studied and compared the brain of man and ape; only a few months ago Professor G. Elliot Smith summarized the result of this intensive enquiry as follows: "No structure found in the brain of an ape is lacking in the human brain, and, on the other hand, the human brain reveals no formation *of any sort* that is not present in the brain of the gorilla or chimpanzee. . . . The only distinctive feature of the human brain is a quantitative one." The difference is only quantitative but its importance can not be exaggerated. In the anthropoid brain are to be recognized all those parts which have become so enormous in the human brain. It is the expansion of just those parts which have given man his powers of feeling, understanding, acting, speaking and learning.

Darwin himself approached this problem not as an anatomist but as a psychologist, and after many years of painstaking and exact observation, succeeded in convincing himself that, immeasurable as are the differences between the mentality of man and ape, they are of degree, not of kind. Prolonged researches made by modern psychologists have but verified and extended Darwin's conclusions. No matter what line of evidence we select to follow—evidence gathered by anatomists, by embryologists, by physiologists or by psychologists—we reach the conviction that man's brain has been evolved from that of an anthropoid ape and that in the process no new structure has been introduced and no new or strange faculty interpolated.

In these days our knowledge of the elaborate architecture and delicate machinery of the human brain makes rapid progress, but I should mislead if I suggested that finality is in sight. Far from it; our enquiries are but begun. There is so much we do not yet understand. Will the day ever come when we can explain why the brain of man has made such great progress while that of his cousin the gorilla has fallen so far behind? Can we explain why inherited ability falls to one family and not to another, or why, in the matter of cerebral endowment, one race of mankind has fared so much better than another? We have as yet no explanation to offer, but an observation made twenty years ago by one on whom nature has showered great gifts—a former president of this association and the doyen of British zoologists—Sir E. Ray Lankester—deserves quotation in this connection: "The leading feature in the development and separation of man from other animals is undoubtedly the relative enormous size of the brain in man and the corresponding increase in its activities and capacity. It is a striking fact that it was not in the ancestors of man alone that this increase in the size of the brain took place at this same period—the Miocene. Other great mammals of the early Tertiary period were in the same case." When primates made their first appearance in geological records, they were, one and all, small-brained. We have to recognize that the tendency to increase of brain, which culminated in the production of the human organ, was not confined to man's ancestry but appeared in diverse branches of the mammalian stock at a corresponding period of the earth's history.

I have spoken of Darwin as a historian. To describe events and to give the order of their occurrence is the easier part of a historian's task; his real difficulties begin when he seeks to interpret the happenings of history, to detect the causes which produced them and explain why one event follows as a direct sequel to another. Up to this point we have been considering only the materials for man's history, and placing them, so far as our scanty information allows, in the order of their sequence, but now we have to seek out the biological processes and controlling influences which have shaped the evolutionary histories of man and ape. The evolution of new types of man or of ape is one thing, and the evolution of new types of motor cars is another, yet for the purposes of clear thinking it will repay us to use the one example to illustrate the other. In the evolution of motor vehicles Darwin's law of selection has prevailed; there has been severe competition and the types which have answered best to the needs and tastes of the public have survived. The public has selected on two grounds—first for utility, thus illustrating Darwin's law of natural selection, and secondly because of appearance's sake; for, as most people know, a new car has to satisfy not only the utilitarian demands of its prospective master but also the esthetic tastes of its prospective mistress, therein illustrating Darwin's second law—the law of sexual selection. That selection, both utilitarian and esthetic, is producing an effect on modern races of mankind and in surviving kinds of ape, as Darwin supposed, can not well be questioned. In recent centuries the inter-racial competition amongst men for the arable lands of the world is keener than in any known period of human history.

The public has selected its favored types of cars but it has had no direct hand in designing and producing modifications and improvements which have appeared year after year. To understand how such modifications are produced the enquirer must enter a factory and not only watch artisans shaping and fitting parts together but also visit the designer's office. In this way an enquirer will obtain a glimpse of the machinery concerned in the evolution of motor cars. If we are to understand the machinery which underlies the evolution of man and of ape, we have to enter the "factories" where they are produced—look within the womb and see the ovum being transformed into an embryo, the embryo into a foetus and the foetus into a babe. After birth we may note infancy passing into childhood, childhood into adolescence, adolescence into maturity and maturity into old age. Merely to register the stages of change is not enough; to understand the controlling machinery we have to search out and uncover the processes which are at work within developing and growing things and the influences which coordinate and control all the processes of development and of growth. When we have discovered the machinery of development and of growth we shall also know the machinery of evolution, for they are the same.

If the simile I have used would sound strange to Darwin's ear, could he hear it, the underlying meaning would be familiar to him. Over and over again he declared that he did not know how "variations" were produced, favorable or otherwise; nor could he have known, for in his time hormones were undreamt of and experimental embryology scarcely born. With these recent discoveries new vistas opened up for students of evolution. The moment we begin to work out the simile I have used and compare the evolutionary machinery in a motor factory with that which regulates the development of an embryo within the womb we realize how different the two processes are. I us imagine for a moment what changes would be necessary were we to introduce "embryological processes" into a car factory. We have to conceive a workshop teeming with clustering swarms of microscopic artisans, mere specks of living matter. In one end of this factory we find swarms busy with cylinders, and as we pass along we note that every part of a car is in process of manufacture, each part being the business of a particular brigade of microscopic workmen. There is no apprenticeship in this factory, every employee is born, just as a hive-bee is, with his skill already fully developed. No plans or patterns are supplied; every workman has the needed design in his head from birth. There is neither manager, overseer nor foreman to direct and coordinate the activities of the vast artisan armies. And yet if parts are to fit when assembled, if pinions are to mesh and engines run smoothly, there must be some method of coordination. It has to be a method plastic enough to permit difficulties to be overcome when such are encountered and to permit the introduction of advantageous modifications when these are needed. A modern works manager would be hard put to it were he asked to devise an automatic system of control for such a factory, yet it is just such a system that we are now obtaining glimpses of in the living workshops of nature.

I have employed a crude simile to give the lay mind an inkling of what happens in that "factory" where the most complicated of machines are forged—the human body and brain. The fertilized ovum divides and redivides; one brood of microscopic living units succeeds another, and as each is produced the units group themselves to form the "parts" of an embryo. Each "part" is a living society; the embryo is a huge congeries of interdependent societies. How are their respective needs regulated, their freedoms protected and their maneuvers timed? Experimental embryologists have begun to explore and discover the machinery of regulation. We know enough to realize that it will take many generations of investigators to work over the great and new field which is thus opening up. When this is done we shall be in a better position to discuss the cause of "Variation" and the machinery of evolution.

If we know only a little concerning the system of government which prevails in the developing embryo we can claim that the system which prevails in the growing body, as it passes from infancy to maturity, is becoming better known to us every year. The influence of the sex glands on the growth of the body has been known since ancient times; their removal in youth leads to a transformation in the growth of every part of the body, altering at the same time the reactions and temperament of the brain. In more recent years medical men have observed that characteristic alterations in the appearance and constitution of the human body can be produced by the action of other glands—the pituitary, thyroid, parathyroid and adrenals. Under the disorderly action of one or other of these glands individuals may, in the course of a few years, take on so changed an appearance that the differences between them and their fellows become as great as, or even greater than, those which separate one race of mankind from another. The physical characters which are thus altered are just those which mark one race off from another. How such effects are produced we did not know until 1904, when the late Professor E. H. Starling, a leader amongst the great physiologists of our time, laid bare an ancient and fundamental law in the living animal body—his law of hormones. I have pictured the body of a growing child as an immense society made up of myriads of microscopic living units, ever increasing in numbers. One of the ways—probably the oldest and most important way—in which the activities of the communities of the body are coordinated and regulated is by the postal system discovered by Starling, wherein the missives are hormones—chemical substances in ultra-microscopic amounts, despatched from one community to another in the circulating blood. Clearly the discovery of this ancient and intricate system opens up fresh vistas to the student of man's evolution. How Darwin would have welcomed this discovery! It would have given him a rational explanation to so many of his unsolved puzzles, including that of "correlated variations." Nor can I in this connection forbear to mention the name of one who presided so ably over the affairs of this association fifteen years ago—Sir E. Sharpey-Schafer. He was the pioneer who opened up this field of investigation and has done more than anyone to place our knowledge of the nature and action of the glands of internal secretion on a precise basis of experimental observation. With such sources of knowledge being ever extended and others of great importance, such as the study of heredity, which have been left unmentioned, we are justified in the hope that man will be able in due time not only to write his own history but to explain how and why events took the course they did.

In a brief hour I have attempted to answer a question of momentous importance to all of us—What is man's origin? Was Darwin right when he said that man, under the action of biological forces which can be observed and measured, has been raised from a place amongst anthropoid apes to that which he now occupies? The answer is yes! and in returning this verdict I speak but as foreman of the jury—a jury which has been empanelled from men who have devoted a lifetime to weighing the evidence. To the best of my ability I have avoided, in laying before you the evidence on which our verdict was found, the rôle of special pleader, being content to follow Darwin's own example—Let the truth speak for itself.

Arthur Keith

EDWARD BRADFORD TITCHENER

The recent death of Professor Edward Bradford Titchener, of Cornell, at the age of sixty, removes one of the most prominent figures in American psychology. Professor Titchener came to this country in 1892, when experimental methods were first beginning to find favor and psychological laboratories were being started in all our leading universities. An Englishman by birth, and a graduate of Brasenose College, Oxford, he studied under Wundt at Leipzig, and had just obtained his doctor's degree when called to Cornell.

On assuming this position Professor Titchener at once adopted a program which has been followed at Cornell ever since. He established a psychological laboratory and made experimental psychology the keystone of the departmental courses. Under his direction, Cornell soon became one of the most productive universities in psychological research. Many of our leading investigators owe their training to Titchener, and the Cornell laboratory has served as model for many departments elsewhere.

While not following Wundt's system in every particular, Professor Titchener held rigidly to the Leipzig ideals. Psychology meant to him introspection by trained subjects or observers, under carefully controlled conditions, with exact measurement of the stimuli and of the observer's responsive activities. He had no sympathy with the behavioristic type of psychology which has grown up in the past fifteen years. For Titchener psychology was the investigation of consciousness—of conscious, subjective experiences. He measured "responses" as a means of obtaining quantitative values for the introspective data; but he did not consider the study of behavior as part of the science of psychology. He set himself the task of analyzing the elementary data of experience—the structure of mind or consciousness—and pursued this analysis systematically throughout his career. The achievements of the Cornell laboratory in this direction are universally recognized by psychologists of every school. No one has challenged the thoroughness nor the scientific accuracy of this work, though certain behaviorists have queried the value of introspective results as contributions to science. The time has not yet come to pass judgment on this question. But the title of Professor Titchener to rank as leader in the analytic or structural investigation of psychology is unassailable. For many years this method and system have been generally known as the Titchenerian psychology.

Titchener's writings are numerous and were always carefully prepared. He is the author of several textbooks on general psychology, both elementary and advanced, the best known being his "Text-book of Psychology" published in 1910. His most important contribution is his "Experimental Psychology," a comprehensive laboratory manual in four volumes (1901-05). Among his works on special topics may be mentioned the "Elementary Psychology of Feeling and Attention" (1908) and "Experimental Psychology of the Thought Processes" (1909). No less important are his editorial contributions. For many years he served as American editor of the English magazine *Mind* (1894–1920), for a time the sole mouthpiece of psychology in Britain. Since 1895 he has been closely identified with *The American Journal of Psychology*, first as associate editor under Stanley Hall (1895-1920), and after Hall's retirement as editor-in-chief (1921–25). To this and other journals he was a frequent contributor of systematic articles, experimental reports, discussions and reviews. The wide range of his contributions is no less remarkable than his clear style and the breadth of his knowledge.

Professor Titchener was an omnivorous reader in the field of psychology. His acquaintance with the older writers extended to medieval and ancient times. He would frequently refer quite incidentally to contributions or hints in some classic source bearing upon a topic on which he or another was working. At the same time he kept fully abreast with current literature. One could not mention in his presence any recent periodical article, however trivial, that he did not show himself perfectly familiar with its contents.

Nor were his interests confined to psychology. He devoted much time to the kindred science of anthropology, and had gathered a large collection of idols, masks, drums and other folk-relics. More recently he developed an interest in numismatics. In connection with this latter avocation he undertook the study of several new languages, including Arabic and Chinese. He was highly appreciative of art in all its forms, particularly music. For a time he served as "professor in charge of music" at Cornell.

In his own field, psychology, there seems to have been a constant conflict between his broad general outlook and his narrower ideals. Professor Titchener's aim was to concentrate the entire research work of his department upon certain definite problems, one topic being taken up at a time, and leading eventually and logically to the next. He was averse to investigation along independent lines by his students and to discussion of extraneous problems in the courses in his department.

This rigid specialization is to-day somewhat exceptional. In most American universities the ideal is to teach a science rather than a system or school. At Cornell the aim was to teach and develop a single type of psychology. This policy has its advantages and disadvantages, both of which have been clearly shown at Cornell. We find on the one hand a splendid body of experimentally obtained contributions to science—on the other hand an increasing lack of sympathy with non-introspective methods of investigation and with the important psychological problems which they suggest.

The same characteristic appears in Titchener's personality. Like Wundt, he preferred to work alone; it was difficult for him to cooperate. He seldom attended the meetings of the American Psychological Association, and for many years withdrew from membership. On the other hand, with his own pupils and his immediate circle of friends he was unreserved and genial. One could always count on him for advice and sympathy. Many years ago he brought together a small group of experimentalists and graduate students from various universities, who were accustomed to meet at various places during the spring recess, to discuss laboratory problems informally and give mutual advice. In these gatherings Titchener ignored all distinctions of age and degree, and treated every one on terms of close intimacy.

The contrast between these two sides of his personality is after all not difficult to understand. Titchener was wholly wrapped up in his work. He had no time to devote to miscellaneous social activities, nor to general meetings, where a large proportion of the papers were quite foreign to his own line of research. But his friends and coworkers were part of his scientific environment, and their interests were closely related to his own. His punctiliousness in certain directions was often misunderstood by those who did not know him and gained for him the reputation of being "difficult." His friends understood him better. They knew that at heart he was sympathetic and thoroughly human, unbending only in matters which seemed to affect his scientific ideals and his standards of conduct. Thoroughly sincere himself, he was deeply offended at anything which seemed to savor of scientific dishonesty. Difference of standpoint had little effect on his friendships, but he was touched to the quick when these differences seemed to result in a lowering of scientific ideals. This distress he covered with a defense reaction of harshness, which was frequently misinterpreted.

It is difficult to estimate at this time Titchener's real place in the development of psychology. But one may safely predict that the value of his extensive experimental contributions will be fully recognized, whatever direction the science may take in the future. It is to be hoped also that Titchener's real personality, the underlying humanity and honesty of the man, may come to be more widely known and appreciated, and that his strict adherence to scientific ideals may have a lasting influence.

HOWARD C. WARREN

PRINCETON UNIVERSITY

SCIENTIFIC EVENTS

MESSAGE FROM THE RETIRING PRESIDENT OF THE BRITISH ASSOCIATION

THE following message from the Prince of Wales, on laying down the presidency of the association, was read at the opening session of the Leeds meeting on August 31:

My year of office as president of the British Association has come to an end, and I can only express my regret to the members of the association, and to our hosts, the City and University of Leeds, that I am unable to attend personally in order to take my leave.

At Oxford last year I ventured in my address to lay before the meeting a view of the relations between science and the state. I felt subsequently some justification for having chosen this topic, when I observed in the proceedings of the Imperial and Colonial Conferences of the past year the extraordinary emphasis laid upon the value of scientific research in relation to imperial development. Both conferences set up special committees on research, and we can not but believe and rejoice that the foundations of an imperial scientific service are being firmly laid. The prime minister of Australia indicated "the application of science both to our primary and secondary industries" as "the most important thing for empire trade"; more recently our ex-president, the Earl of Balfour, invited the attention of the House of Lords to "the enormous value of the work given by men of science, with the most lavish generosity," to the study of problems of the common welfare.

Such events as these place it beyond doubt that one of the main objects of the British Association itself is in process of achievement, namely, that of "obtaining more general attention for the objects of science." The association, the so-called parliament of science, is one of the chief instruments to that end, and I trust that the public support will continue, in increasing measure, to be accorded to its work. Its powers, I am happy to say, have been very materially strengthened, during my own term of office, through the splendid generosity of Sir Alfred Yarrow, in making a gift of £10,000 for the general purposes of the association, to be expended, in accordance with his wise provision, in the course of twenty years. I gladly take this opportunity of publicly repeating the thanks of the association to Sir Alfred Yarrow.

In resigning the chair to Sir Arthur Keith, I can wholeheartedly congratulate the association on its choice of my successor. His name stands very high in the science of man's origin and early biological history. I have reason to believe that when any one in this country digs up a bone his first instinct (subject to the intervention of the police) is to send it to Sir Arthur Keith. You are to hear from him an address on Darwinism as it stands today—a subject of perennial interest, and more than once one of warm controversy at our own meetings. The occasion of the presidential address does not (I am thankful to say) lend itself to controversy, but the warmth I am sure you will supply in your welcome to Sir Arthur Keith, and, meeting as you are in Leeds, that warmth will be increased by the traditional quality of Yorkshire hospitality.

THE FIFTH INTERNATIONAL GENETICS CONGRESS

As has already been recorded here, the Fifth International Genetics Congress will be held in Berlin from September 11 to 17, with headquarters at the University of Berlin. The general program will begin with a visit to the Zoological Gardens at five o'clock on Sunday afternoon and a reception at seven at the restaurant in the gardens. Following is the program of general sessions for the week:

Monday, September 12—Opening session (Address of welcome, Election of the Presiding Committee). Address: R. v. Wettstein, Vienna: Das Problem der Evolution. Evening: Informal reception by the Reichsregierung and the Preussische Staatsregierung.

Tuesday, September 13—General meeting. Addresses by R. Pearl, Baltimore: Eugenics; O. Rosenberg, Stockholm: Speziesbildung mit Vervielfältigung von Chromosomen, and H. Federley, Helsingfors: Chromosomenverhältnisse bei Mischlingen. Evening: Reception by the Municipal Government of Berlin, in the Rathaus.

Wednesday, September 14—General meeting. Addresses by A. Pézard, Paris: Hormones sexuelles et hérédité mendélienne chez les Gallinacés; N. I. Vavilov, Leningrad: Geographische Genzentren der kultivierten Pflanzen, and A. F. Blakeslee, Cold Spring Harbor: Genetics of Datura. Evening: Special performances in the Staatlichen Opernhaus and the Städtischen Oper.

Thursday, September 15—General meeting. Addresses by C. Correns, Dahlem: Nichtmendelnde Vererbung; H. J. Muller, Austin: The problem of genic modification, and H. Winkler, Hamburg: Zur Theorie der Crossing-over-Erscheinungen. Afternoon: Visit to the institutes at Dahlem.

Friday, September 16—General meeting. Addresses by F. A. E. Crew, Edinburgh: Organization and function of an animal-breeding research department, and J. Seiler, München: Die Geschlechtschromosomenfrage. Afternoon: Excursion to Potsdam and Sanssouci.

Saturday, September 17—Business meeting to determine the next meeting-place and to elect the committee for the preparation of the next congress. Evening: Closing dinner in the Zoologischer Garten.

Divisional meetings have been arranged as follows: (1) General Genetics, with 57 papers; (2) Cytology and Genetics, with 20 papers; (3) Genetics of cultivated plants, with 16 papers; (4) Genetics of domestic animals, with 8 papers; (5) Human Genetics, with 15 papers, and (6) Eugenics, with 9 papers.

Americans contributing to the congress are: Dr. Chas. B. Davenport, Dr. E. C. MacDowell, Dr. M. Demerec, Dr. A. M. Banta and Dr. Th. R. Wood, of Cold Spring Harbor; Professor H. E. Crampton and Dr. L. J. Stadler, of Columbia University; Professor E. M. East, of Harvard University; Professor W. H. Eyster, of the University of Maine; Professor E. W. Lindstrom, of Iowa State College; Professor F. B. Hanson, of Washington University; Professor G. H. Shull, of Princeton University; Professor Charles Zeleny, of the University of Illinois; Professor R. E. Cleland, of Goucher College; Dr. K. Sax, of the Maine Agricultural Experiment Station; Professor N. E. Hansen, of the South Dakota College; Professor Leon J. Cole, of the University of Wisconsin; Dr. L. C. Dunn, of the Connecticut Agricultural College; Dr. W. S. Anderson, of the University of Kentucky; Professor R. E. Clausen, of the University of California; Dr. C. J. Lynch, of the Rockefeller Institute; Dr. P. W. Whiting, of Boston; Dr. O. E. White, of the Brooklyn Botanic Garden; Professor Raymond Pearl, of the Johns Hopkins University; Professor A. F. Blakeslee, of Cold Spring Harbor; Professor H. J. Muller, of the University of Texas.

Several excursions of special interest to geneticists are planned at the conclusion of the congress.

THE AMERICAN CHEMICAL SOCIETY

The American Chemical Society will hold its seventy-fourth meeting at Detroit, beginning on September 5.

The general program is as follows:

Monday, September 5

10:00 A.M.—Registration, Lobby of Ball Room, Statler Hotel.

2:00 P.M.—Council Meeting (continued in evening if necessary).

8:30 P.M.—Informal Reception and Dance.

Tuesday, September 6

11:00 A.M.—General Meeting, Statler Hotel Ball Room.

Addresses of Welcome:

In the name of the Detroit Section: L. W. Rowe, Chairman, Detroit Section.

In the name of the City: Mayor John Smith.

Response:

George D. Rosengarten, President, American Chemical Society.

1:30 P.M.—Ladies' trip to Bonstelle Playhouse.

2:00 P.M.—General Divisional Meetings:

Agricultural and Food, Biological, Chemistry of Medicinal Products, and Dye Divisions.

Industrial and Petroleum Divisions, Joint Symposium on "Chemistry Contribution to Automotive Transportation."

Physical and Inorganic Divisions, Symposium on "Present Status of the Chemistry of Proteins."

Organic Division—Small Banquet Room, Statler Hotel.

8:30 P.M.—Boat ride with special entertainment features.

Wednesday, September 7

9:30 A.M.—Divisional Meetings.
1:30 P.M.—Ladies' Trip. Luncheon and Bridge at Detroit Boat Club.
2:00 P.M.—Divisional Meetings.
8:00 P.M.—Public Meeting and President's Address.

Addresses:

George D. Rosengarten, President, American Chemical Society, "Reflections."

Charles F. Kettering, "The Functions of Research."

Thursday, September 8

9:30 A.M.—Divisional Meetings.
1:30 P.M.—Visits to Manufacturing Plants. Trip (1) Ford, River Rouge; Trip (2) Ford, Highland Park; Trip (3) Sight-seeing.
2:00 P.M.—Golf Tournament.
6:30 P.M.—Group Dinners.
9:00 P.M.—Special Feature Entertainment.

Friday, September 9

8:00 A.M.—Visits to Manufacturing Plants. Trip (4) Parke, Davis & Co.; Trip (5) U. S. Rubber Co.; Trip (6) Acme White Lead and Color Works.
9:30 A.M.—Divisional Meetings (if scheduled).
11:00 A.M.—Trip (7) Ann Arbor.
2:00 P.M.—Trip (8) Dodge Brothers Motor Car Co.; Trip (9) Packard Motor Car Co.; Trip (10) Cadillac Motor Car Co.

Saturday, September 10

There are many interesting boat, interurban car, bus, auto or airplane rides for those who have the time and inclination.

SCIENTIFIC NOTES AND NEWS

Dr. W. B. Cannon, of the Harvard Medical School, has been made chairman of the committee of arrangements for the International Physiological Congress to be held in Boston in 1929.

Dr. Fridtjof Nansen, professor of oceanography in the University of Oslo, has been elected a corresponding member of the Prussian Academy of Sciences in the section of mathematical physics.

Dr. C. Correns, director of the Kaiser-Wilhelm Institute for Biology, has been elected an honorary member of the Botanical Society of Tokyo.

The Russian Academy of Sciences has elected as a corresponding member Professor James Franck, of the University of Göttingen, as recipient of the Nobel prize in physics. Professor Albert Einstein and Professor Walther Nernst, of Berlin, who were already corresponding members, have been elected honorary members.

Honorary doctorates have been conferred by the University of Innsbruck on Dr. Heinrich Herkner, Dr. Karl Heider and Dr. Albrecht Penck, all of the University of Berlin.

The Ling Foundation of Los Angeles has awarded a gold medal to Dr. Michael S. Creamer for work in behalf of the health of the school children of southern California. The Ling Foundation was recently organized to forward child health work in southern California.

George Higgins Moses, United States Senator from New Hampshire, has been made chairman of a special committee authorized by the 1927 Legislature to investigate the feasibility of improving marsh lands at Hampton and to find a remedy for coast erosion.

Dr. F. G. Cottrell, who since September, 1922, has been director of the Fixed Nitrogen Research Laboratory, will continue in charge of this work as chief of fertilizer and nitrogen fixation investigations in the new Bureau of Chemistry and Soils. In addition to fixed nitrogen, this unit will include phosphoric acid, potash and fertilizer investigations being made at the Arlington Experiment Farm.

Dr. F. A. Ernst, acting chief of the fertilizer and nitrogen fixation investigations and for some time a member of the Fixed Nitrogen Research Laboratory staff, has resigned to join the engineering staff of the Atmospheric Nitrogen Corporation.

Dr. Howard R. Moore, formerly with the Eastman Kodak Company, has been appointed to the staff of the Cryogenic Laboratory of the Bureau of Standards.

William Henry Patchell, British consulting engineer and past-president of the Institution of Mechanical Engineers of Great Britain, recently arrived in the United States. Mr. Patchell acts in the capacity of consulting engineer for a number of American public utilities.

Dr. W. A. Setchell, of the University of California, has returned from a tour around the world. He made a special study of coral reef formation among the islands of the Pacific for a period of four months, and also made a survey of varieties of subantarctic algae. A number of valuable collections made during the trip will be housed in the department of botany.

Dr. R. M. Wenley, who has been acting as director of the British Division of the American University Union, returns next month to the headship of the department of philosophy and psychology in the University of Michigan.

Dr. J. B. Austin, of Yale University, as a guest of Cryogenic Laboratory of the Bureau of Standards, has been engaged in an investigation of the ultra-violet absorption spectra of toluene and the three xylenes at low temperature.

Frank Reeves has been granted leave from the U. S. Geological Survey for four months to do commercial work in petroleum geology in Canada.

Two series of colloquia will be given at the University of Wisconsin summer meeting of the American Mathematical Society and the Mathematical Association of America, held during the week of September 5. Professor Anna Pell-Wheeler, of Bryn Mawr College, will lecture on "The Theory of Quadratic Forms in Infinitely many Variables and Applications." Professor E. T. Bell, of California Institute of Technology, will lecture on "Algebraic Arithmetic."

Louis A. Fuertes, distinguished as a naturalist and artist, lecturer at Cornell University, was killed in an automobile accident on August 21. Mr. Fuertes was born in Ithaca, New York, on February 7, 1874.

Professor Lionel Remond Lenox, for thirty-five years a member of the faculty in chemistry at Stanford University, died on July 25, aged sixty-two years.

John Henry Reynolds, formerly dean of the faculty of technology of the University of Manchester, died on July 17, at the age of eighty-five years.

C. W. Daniels, formerly director of the London School of Tropical Medicine, died on August 6, aged sixty-five years.

Professor Alexander Backhaus, formerly director of the Agricultural Institute of the University of Königsberg, has died at the age of sixty-one years.

A new episcope has been installed at the Royal Society of Medicine, London, as part of the memorial to the late secretary, Sir John MacAlister. The instrument has been constructed in Germany by Carl Zeiss, according to the specifications of the committee in charge of the memorial, on whose behalf the secretary of the society, Mr. G. R. Edwards, accompanied by the operator, made a special journey to Jena and spent four days in the Zeiss workshops conferring with experts of the firm.

Memorial Hospital, New York City, an institution for the study and treatment of cancer and allied diseases, has received $25,000, or $5,000 a year for five years, from Lucius N. Littauer, of Gloversville, N. Y., to defray the cost of special research in chemo-therapy; $5,000 from Mrs. S. M. Gibbons, of New York, to finance the study of the "gross clinical cases of cancer," and $2,500 from Daniel Guggenheim, of New York, for special research on the effects of radioactivity on certain parts of the body. It will be remembered that Memorial Hospital received recently for the enlargement of its work $60,000 a year for five years from Mr. John D. Rockefeller, Jr., and $250,000 for the purchase of radium from Mr. Harkness.

The fourth International Congress of Theoretical and Applied Limnology will be held in Rome this year, and a full program has been arranged covering the period from September 18 to October 3. From *Nature* we learn that the congress will be divided into four sections, dealing with physics and chemistry, geology and hydrography, biology, and applied limnology, respectively. The first week will be spent in Rome, during which time lectures and papers will be given and opportunity will be afforded for visiting the Limnological Exhibition, the Royal Central Laboratory of Hydrobiology, the Royal Fish Breeding Establishment and the Zoological Gardens. Receptions will also be given on the Capitol and at the Royal Italian Geographical Society. Two days will be spent at Naples, where the visitors will be received at the zoological station and at the university. The congress will conclude with an itinerary to the Hydrobiological Stations on Lakes Garda, Como and Maggiore, while nights can be spent *en route* at Perugia, Verona and Milan. The congress will end on Monday, October 3, at Lake Maggiore.

The Archivio di storia della scienza, which was started in 1919 for the history of science under the editorship of Aldo Mieli, of Rome, is now being enlarged and issued as an international journal, in which articles appear in the Italian, English, French or German language. Twenty-seven foreign editorial collaborators have been chosen. The two thus far representing the United States are Florian Cajori, of the University of California, and Edgar F. Smith, of the University of Pennsylvania.

In the development of the Blandy Experimental Farm of the University of Virginia, which was bequeathed to the university by the late Graham F. Blandy, the appointment has been recently announced of Dr. Orland E. White, curator of plant breeding and economic plants at the Brooklyn Botanic Garden, as professor of agricultural biology and director of the farm. *The Experiment Station Record* reports that five research fellowships have been established, two of \$1,000 each and three of \$500 each, to which graduates from standard colleges who have majored in biology or agriculture will be eligible. Appointees are expected to register in the graduate department of the university and take work leading toward a higher degree.

THE U. S. Public Health Service has called attention to the unusual prevalence of poliomyelitis. Reports from eight states disclosed the presence of 192 cases of this disease for the week ended August 6. Incomplete reports from 34 other states showed only 42 cases. For the week ending August 6, California reported 56 cases; Connecticut, 11; Massachusetts, 10; Missouri, 15; New Jersey, 17; New Mexico, 9; Oklahoma, 3; Texas, 10. For the corresponding week of last year, 38 states reported only 66 cases.

The South African Mining and Engineering Journal contains notice of the discovery by H. R. Adam, of a new palladium mineral from the Tweefontein workings, having the apparent formula Pd_3Sb. The mineral contains practically no platinum, and the sperrylite $PtAs_2$, found in the same locality, is free from palladium. These two minerals represent the only compounds of the two metals found in nature.

RICHARD M. SUTTON, of the Norman Bridge Laboratory of the California Institute of Technology, writes: "At 8:40 on the evening of July 25, while riding through the San Joaquin Valley near Merced, my attention was attracted by a brilliant meteor falling toward the south. As it approached the earth it suddenly broke into two pieces of nearly equal intensity, whose paths diverged at an angle of 30° from each other. After continuing their flight for about one second longer, they were both extinguished at approximately the same time."

DR. T. D. A. COCKERELL writes to SCIENCE as follows: "We reached Leningrad July 10 on the Russian steamer *Soviet,* having three days at Bremen on the way. We had three days in Leningrad and saw the principal scientific establishments. The museum of the Academy of Sciences is magnificently full of important things, all excellently arranged. We found the entomological collections very extensive and in very good order. We saw the famous mammoth, with hair on it, dug out of the ice in Siberia; also the last meal from its stomach. The academy collections came quite safely through the revolution, thanks especially to Professor Karpinsky. The Botanic Garden is very fine; we were shown everything by Professor Komaroff, well known for his studies of the Asiatic flora. The great building and extensive operations of the geological committee astonished us. Here we saw the duck-billed dinosaur found on the Ameer, very like Canadian forms. On the way here we had part of a day in Moscow, where we were taken about by Professor D. Itovaisky, well known for his work on ammonites. At Irkutsk we are comfortably placed in the guest-room of the Geological Committee. We have already been to Ust Balei for fossil insects, and have made good collections of recent insects. Tomorrow we expect to go to Lake Bailkal. We have seen a great deal of Professor W. Schewiakoff of the university here. His work on radiolaria is, I presume, much the best ever done on that group. The university here, though only a few years old, is already an important institution, and supports a research laboratory on Lake Bailkal. We are the first Americans to see it in its developed condition, and to see Professor Schewiakoff's very beautiful preparations showing the anatomy of various animals. Every one without exception has been very kind to us. There is a general desire for more intercourse with America."

THE American Ceramic Society is sponsoring a foreign trip in 1928. The tour has been built around European ceramic centers such as: Stoke-on-Trent and the "Potteries" in England, Delft in Holland, Meissen in Germany, Prague in Czecho-Slovakia and Paris in France.

THE annual inspection by the General Board of the British National Physical Laboratory at Teddington took place on June 24. Over 1,000 visitors were conducted over the extensive buildings, and a keen interest was shown in the progress of the various departments where the staff were engaged carrying out their work of research and experiment. The visitors were received by Sir Ernest Rutherford, president of the Royal Society and chairman of the board, Sir Richard Glazebrook, chairman of the executive committee, and Sir Joseph Petavel. In the aerodynamics department special attention was centered on the Cierva autogyro, which was described as a wingless aeroplane. In the engineering department instruments for measuring and recording the vertical and horizontal disturbances of the ground due to traffic received much attention. These instruments have been designed and constructed at the laboratory primarily for the measurement of ground disturbances due to road and rail traffic. Particularly interesting was a working model of a motor-car showing the action of the front and back brakes. It revealed that the sudden application of the back brake made the vehicle swerve to one side or the other, while the application of the front brake only made the car skid straight on. Interesting experiments were also witnessed in the metallurgy and physics departments.

It is announced in *Nature* that the British Secretary of State for the Colonies has appointed a committee "to formulate practical proposals for submission to the Colonial Governments to give effect to the resolution for the Colonial Office Conference on the subject of Colonial Agricultural Scientific and Research Services." These proposals are to "include a scheme, based on contributions to a common pool, for the creation of a Colonial Agricultural Scientific and Research Service available for the requirements of the whole Colonial Empire for the support of institutions needed for that purpose, and for the increase of research and study facilities in connection with specialist services of the Colonies generally." The committee is thus constituted: Lord Lovat, Parliamentary Under-Secretary of State for Dominion Affairs (chairman), Mr. W. Ormsby-Gore, Parliamentary Under-Secretary of State for the Colonies; Sir Graeme Thomson, Governor of Nigeria; Mr. A. S. Jelf, Colonial Secretary, Jamaica; Mr. O. G. R. Williams, Assistant Secretary, Colonial Office; Major R. D. Furse, Private Secretary (Appointments) to the Secretary of State for the Colonies; Sir J. B. Farmer, Dr. A. W. Hill, Mr. F. L. Engledow, and Dr. A. T. Stanton, Chief Medical Adviser to the Secretary of State for the Colonies.

The *British Royal Geographical Journal* reports that the Italian National Committee for Geography has decided to promote the creation of a fund for studies in Palestine, which will be devoted mainly to geographical research, whereas the scope of similar institutions which have existed for many years in other countries has been mainly historical and archeological. In accordance with this purpose, the first work to be undertaken by the Italian fund will be an expedition to chart the Dead Sea on a fairly large scale and to map the adjoining portion of the depression in which it lies and of the Jordan trough. In addition to the hydrography and mapping a geological survey and various limnological researches will be carried out. It is also planned to establish a station on the Dead Sea shores for protracted observation of variations in the sea-level and of the meteorological conditions of the basin.

UNIVERSITY AND EDUCATIONAL NOTES

The will of Elbert H. Gary, chairman of the board of the United States Steel Corporation, includes bequests for scholarship funds of $50,000 each to McKendree College, the University of Pittsburgh, Lafayette College, Trinity College, Lincoln Memorial University, Syracuse University, Northwestern University and New York University.

The campaign to raise $1,000,000 for the Medical School of Howard University, Washington, has been brought to a successful conclusion. Negroes contributed $150,259 to the campaign. A tablet bearing the names of fifty-one Negroes whose donations ranged from $1,000 to $10,000 will be placed in the new medical school building.

At a meeting of the board of trustees of Ohio State University, on August 4, Dr. John H. J. Upham, Columbus, was appointed acting dean of the Ohio State University College of Medicine. Dr. Eugene F. McCampbell, who was formerly dean, retired on July 1.

Dr. Albert Warren Stearns has been appointed dean of Tufts College Medical School, Boston, and will take up his new work on September 1, succeeding Dr. Stephen Rushmore.

Dr. F. M. Baldwin, who for the past ten years has been in charge of physiology at Iowa State College at Ames, has resigned his professorship to take charge of the department of physiology and become director of experimental marine biology in the University of Southern California at Los Angeles.

M. N. Short, of the U. S. Geological Survey, has been appointed lecturer in mining geology at Harvard University during the absence of Professor Graton on his sabbatical year.

Dr. O. H. Elmer, assistant plant pathologist at the Iowa Agricultural Experiment Station, has been appointed to succeed Dr. R. P. White as assistant professor of botany and assistant plant pathologist at the Kansas College and Experiment Station.

D. S. Masters, of Ohio State University, has been appointed instructor in chemistry and chemical engineering at Washington University, St. Louis, Mo.

Dr. J. H. Ashworth, of the University of Edinburgh, has been transferred from the chair of zoology to the chair of natural history.

DISCUSSION AND CORRESPONDENCE

THE "WASHBOARD" OR "CORDUROY" EFFECT DUE TO TRAVEL OF AUTOMOBILES OVER DIRT ROADS

The writer returned recently from a somewhat extended trip by motor car in the Mojave desert, where much of the mileage was over unpaved roads. Of these, two types were noted, first, primitive desert road, which winds among sage and cactus over the long gently sloping alluvial washes characteristic of this desert, apparently following the trail of the first wagon or automobile to mark the way, and second, the worked dirt roads mainly traveled.

The first type, particularly serving as feeders for ranches lying some miles off the highway, consists mostly of the two ruts of the wheels, usually sandy and gravelly, underlain by more compact, or solid material. In this type of unpaved road the "washboard" effect to be described was not observed to a very appreciable extent. It is surprising how rapidly automobiles can be driven on such roads by drivers accustomed to them, with the many turns, which in places are quite abrupt, and with the necessity of keeping in the ruts. As much as a fifty-mile rate has been averaged on such a road, according to reliable information. With the continued twisting of the road the driver must continually watch details of the steering, so as not to let the wheels cut into the sand and gravel of the walls of the ruts. The driver unaccustomed to this type of road tends to turn his steering-wheel too soon in approaching a turn, causing departure from the ruts, whereas the trick is to let the wheels ride the ruts clear to their turning, and to turn with them. Of course much of the success of rapid driving under these conditions is in knowing from experience the details of the road, such as the curvature of each turn, its total angle, and where the straighter stretches are located that will permit speeding.

On the other type of dirt road, graded and occasionally dragged, the "washboard" phenomenon was found frequently. It may be familiar to drivers on dirt roads everywhere. A stretch of road well illustrating it at intervals is that between Daggett and Needles on the much-traveled National Old Trails Highway (Santa Fé Route). The usual road surface on this stretch is a rather thin layer of loose sand and gravel, or small fragments of broken rock, lying on more compact material as base. The phenomenon consists in the heaping up, by the tires of the automobiles themselves, of this loose material into parallel ridges, an inch or so in height, crossing the road at right angles, often with a slight sinuosity. The ridges have a quite uniform spacing of one and one half feet, very roughly, and are found in groups of six or eight, or more. A remarkable fact about them is that while as a group they appear to have grown in the direction of the road, as though they were the effect of sheets of water flowing along on the road, their growth has been essentially across the road, and is the result of the passing of many machines in both directions.

A reasonable explanation of the "washboard" roads ("corduroy," they are sometimes called, from a similarity to the old time roads where logs were laid transversely to make a safe passage for vehicles on soft ground) would seem to be as follows: Given, a somewhat shallow and uniform layer of loose sand and gravel on a fairly flat, hard surface, and a local inequality, say a fair-sized rock, or a chuck-hole in the solid base, which imparts a sudden upward impulse to one or both of the front wheels of the first automobile to encounter it. Consider one of the front wheels as experiencing this bounce. The physical quantities involved are (1) the elasticity both of the tire and of the spring, and (2) the inertia of the wheel system as distinct from that of the much heavier body. The necessary conditions are thus present for vibrations. If the automobile is supposed to be traveling at a speed of 35 or 40 miles per hour, the body of the car, on account of its far greater inertia, may be assumed to undergo at the time of the bounce a vertical displacement which is relatively negligible. With a given elasticity in the system the particular wheel mentioned will have a definite period of vertical vibration relative to the approximately non-displaced body. Thus it will vibrate for an appreciable time after the initial shock, and each time it moves away from the body of the car it will squeeze out before and behind it on the road some of the loose material, and in this way leave a series of equally spaced low ridges and shallow depressions. It is conceivable that a sympathetic effect would at the same time be transmitted to the other front wheel, which also would then tend to produce a similar effect on the loose material over which it passes. There must follow similar action on the part of the rear wheels. The question arises as to how the natural frequency of the front wheels, with respect to the body of the car, compares with that of the rear wheels. The two periods must be comparable, otherwise the rear wheels would tend to undo the work of the front ones, with the result that the well-defined washboard effect would not perpetuate itself.

An explanation offered by a driver living in the desert, of the phenomenon, was that the wheel, at the first bounce, because of lowered resistance between tire and road resulting from the diminished pressure between them, actually "spins," and in doing so kicks some of the loose material backward, repeating this action a number of times as the bouncing, that is, the vibration, continues. This explanation would require, at any rate after the group of ridges is once established, that the diminished pressure occur when the wheel is nearest the more compact sub-surface of the road, if the ridges are to perpetuate themselves in positions approximately fixed, so that the pressure should then be greater on the ridge than in the depression. This greater pressure on the loose material of the ridge would then be expected to show a tendency toward flattening it out, undoing the effect of the wheel while "spinning" in the depression. However, if the wheel slips periodically at the depressions but not at the ridges, then for a constant speed of the car body forward over the road, and thus presumably of the axle also (neglecting any vibration lengthwise of the road, of the wheels relative to the body) a constant rate of rotation of the wheels would be consistent with this explanation. For in ascending and descending the ridges the periphery of the tire at the point of contact would be moving at an angle with the direction of the forward motion of the axle, and if there were no slipping anywhere, would have to be moving circumferentially around the axle at a rate greater than that necessary in the depressions. But by the same argument there would be a tendency to spin on the peaks of the ridges also, which would tend to wear them down. This explanation by slippage uses, to be sure, the natural rate of vibration of the wheels relative to the approximately stationary body of the automobile. But on its face it does not appear so acceptable as that attributing the ridges entirely, or almost so, to the simple, periodic bumping of the road by the wheels, with negligible periodic slip.

The problem more generally, and in its simplest form, is that of a vibrating system of two masses, one much greater than the other, connected by an elastic spring, and affected by an elastic push corresponding to that of the rubber tire, and by gravity.

As to the transverse growth of the ridges, whose individual identity as they lie across the road, can be recognized often over a length of ten or twelve feet, possibly more, suppose a second car, similar to the first, follows it and strikes the first humps formed, which will be necessarily short transversely to the road. The chances are that the wheel of the second car will not strike the humps squarely if at all, but at one end of them or the other. But this is sufficient to give an initial bounce, and the result is a slight lengthening, transverse to the road, of the initial humps. It is rather remarkable that the two series of humps, one at either side of the first car to pass, should eventually join up into continuous ridges across the road, but this is the actual effect from the passing of many cars. Perhaps this joining up is the result mainly of a sympathetic action, mentioned above, of the wheel on the opposite side of the machine.

Alternate grooves and ridges, roughly parallel, can be formed by water flowing across the road, but this type is likely to lie obliquely rather than perpendicularly with respect to the road, and at any rate will hardly be so uniformly spaced over a considerable distance. The opinion may be ventured that where ridges of the particular washboard type are found on solid road surface devoid of loose material, they were formed by the vibration process at a time when the ground, due probably to moisture, was in a more pliable condition.

The frequency of vibration of the wheels of a car relative to a stationary body is a quantity much greater than the frequency of the heavier body relative to stationary wheels. Assuming one and one half feet as the approximately uniform interval between the ridges of the "washboard," and further assuming 30 miles per hour as the speed of the "average" car, the average vibration rate of the wheels relative to the body of the car, comes out about 30 v.p.s. ($v = \lambda$. The value of one and one half feet as the distance between ridges in a group makes the vibration rate about 2 per cent. less than the speed of the automobile in miles per hour.) A certain driver, who has driven much on these desert roads, mentioned 25 miles as an average value for all machines, which would give approximately 25 v.p.s. for an average value.

The question might be raised whether the "average" car, or a particular class of cars, heavy or light, is in the main responsible for the ridges. Also, is the vibration chiefly that of the balloon tires? Heavier cars with balloon tires were observed to travel in the straight stretches at 40 miles and better. Riders in the heavier machines traveling at the higher speeds are probably little disturbed by these corrugations on the road. The bumping effect in a lighter car at a speed of 12 or 15 miles would become at times monotonous, to say the least. A certain other driver living in the desert stated he found the bumping effect least at a speed of about 35 miles. This should vary with the type of automobile, but theoretically for each car there is one best speed for maximum comfort of riding, and that is the speed at which the wheels "resonate" with the ridges.

The writer has taken no actual measurements on these ridges. Moreover, it would be interesting to check the vibration rates of automobile wheels in the laboratory, with the values calculated on the basis of the physical explanation offered. Such matters are properly subjects for consideration in the fields of road and automobile engineering.

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THE REVERSIBLE MIXING OF SUBSTANCES IN THE CONDENSED STATE AT THE ABSOLUTE ZERO OF TEMPERATURE

The thermodynamical results established in a previous paper[1] and extended in a subsequent paper[2]

[1] Read at the Philadelphia meeting of the American Association; Science, Feb. 25.

were further extended and applied in a paper read at the Washington meeting of the Physical Society. An outline of a few of the more important results may be of interest to the readers of SCIENCE on account of their bearing on physico-chemical experiments frequently performed in a laboratory, and involving quantities often made the subject of accurate determinations.

It is shown that the internal heat of mixing h_m, or the increase in internal energy on mixing a number of substances, is zero, or

$$h_m = O$$

at the absolute zero of temperature, if the substances and resultant mixture are under the pressures of their vapors. It is also shown that

$$\frac{dh_m}{dT} = O$$

$$\text{and } \frac{d^2h_m}{dT^2} = O$$

where T denotes absolute temperature. Hence if h_m can be expanded in powers of T by Taylor's Theorem

$$h_m = aT^3$$

near the absolute zero of temperature, where a is a constant. This result could be investigated experimentally without great difficulty. It would involve measurements of the change in temperature on mixing a number of substances near the absolute zero of temperature, and a determination of the corresponding specific heats of the substances and the resultant mixture. The quantities H_m and A are shown to possess similar properties, where H_m denotes the heat absorbed on reversibly mixing the substances and A the maximum work done during the process.

In the first paper on the subject it was shown that the controllable internal energy and entropy, which are functions of the controllable variables v and T, are zero for any substance or mixture in the condensed state under their vapor pressures at the absolute zero of temperature. If several substances are simultaneously considered another controllable operation becomes possible, namely that of mixing some of them. From the way the foregoing result was established it does not follow directly that there will be no change in internal energy or entropy on mixing the substances under their vapor pressures at the absolute zero of temperature. It is now shown that no change takes place. With this result as basis it is further shown that the well-known formulae

$$\Delta U = h_m$$

$$T \Delta S = \Delta U + A = h_m + A = H_m$$

$$\Delta U = T\left(\frac{\partial A}{\partial T}\right)_v - A$$

hold also if U and S represent the controllable internal energy and entropy respectively. Since these quantities can be calculated from experimental data a method is afforded of testing the truth of the method of deduction of the various results obtained, and also of testing the truth of the first and second law of thermodynamics on which all the results are fundamentally based.

R. D. KLEEMAN

SCHENECTADY, N. Y.

[2] Read at the New York meeting of the Physical Society; SCIENCE, April 29.

DOUBLE COVEY OF CALIFORNIA VALLEY QUAIL

IT is common knowledge that the males of many species of birds assist in the protection and care of the young birds. During the week of June 12–18, the following interesting observations were made by Mr. R. A. Holley, of Fillmore, California, on what was apparently a double covey of California Valley quail or partridge (*Lophortyx californicus vallicola* (Ridgw.)). In the early part of the week he flushed a large flock of quail in an orchard. The covey consisted of twenty-three young quail of two distinct sizes and two adult males, one of which had a crippled leg, but no adult females. Approximately one half of the young quail were about one third grown, the rest were of uniform size but somewhat larger.

The following day the same covey was seen again. The crippled male was acting as sentinel while the other male was feeding with the young ones. When the sentinel was approached the covey flew a short distance away. It was then noted that the crippled male had taken his place with the young on the ground and that the other male was acting as the sentinel from the fence post. This same covey of two males, one a cripple, and the twenty-three young belonging to two size groups were seen on four successive days in the same orchard. Apparently the females of the two adult pairs had been killed and the two males with their respective broods had joined forces. This alliance had made it possible for the males to alternate as sentinels and warn the combined broods of any impending danger.

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SCIENTIFIC BOOKS

Man not a Machine. By E. RIGNANO. London. Kegan Paul, French, Trubner & Co., 1926. 77 pp.

IN this handy little volume Rignano discusses in a brief but suggestive way the mechanistic and the vitalistic interpretations of life, especially of the life of man. The subject matter is considered under nine heads, such as metabolism, adaptation, behavior, instincts, mentality, social relations, and the like. The author concludes that in all nine aspects there is an irreducible residuum that can not be explained away on mechanistic grounds. This irreducible element, always present, is of a purposive character. Having thus shown the insufficiency of the mechanistic interpretation, Rignano concludes that a vitalistic interpretation of life is the only one tenable. To the reviewer this step seems to be a *non sequitur*, for in addition to vitalism and mechanism there are other possible ways of considering life, witness that embodied in emergent evolution. Thus the view of life from the standpoint of emergent evolution avoids the obvious limitations of the mechanistic conception and yet differs radically from vitalism. It may be, therefore, a much more truthful interpretation of life than either vitalism or mechanism. It is to be regretted that this aspect of the subject has not been discussed by Rignano, whose book, however, affords good reading, suggestive and stimulating.

G. H. Parker

Traité de Geographie Physique par Emmanuel de Murtonne, professeur à la Sorbonne. Tome troisième: Biographie (en collaboration avec A. Chevalier et L. Cuénot) Un Vol. in 8°, 464 pages, 94 figures dans le texte, 24 photographies hors texte. Librairie Armand Colin, Paris.

The first edition of the "Traité de Geographie Physique" appeared twenty years ago and a second edition later. The author has remodeled his work, which has now been published in a third edition. Volume III devoted to biogeography completes the work, and in it there are 404 pages of text, instead of 154 pages in the first edition, 94 figures in place of 62, and 25 pages of bibliography instead of 10 pages. The growing complexity of the subject, and the abundance of technical studies devoted to biogeography have been such as to necessitate the association of two other scientists: MM. Chevalier, director of the laboratory of colonial agronomy, and Cuénot, professor of zoology in the University of Nancy. The volume is a single complete treatise on biogeography and is based on current and recently pursued research on the subject. A chapter is devoted to general principles, as common to botanical and zoological geography.

Five chapters are devoted to phytogeography. One of them deals with the science of the soil, another to plant sociology, where are given in a detailed manner the most recent investigation of plant associations and their evolution. Another important chapter considers the influence of man on vegetation with an essay on the classification of the systems of cultivation.

Three chapters deal with zoogeography and are filled with matters of great interest to zoologists, such as the origin of species and their adaptation to diverse surroundings. For geographers, this book is a mine of information. It ought to appeal to agriculturists, economists, colonial experimenters and the public in general.

John W. Harshberger
University of Pennsylvania

SCIENTIFIC APPARATUS AND LABORATORY METHODS

ACCURATELY TIMED INTERMITTENT LIGHTING

In many types of biological work a dependable, home-made apparatus for providing accurately timed alternate periods of light and darkness is desirable. Commercial machines are generally so high priced as to be out of the question in small laboratories.

The apparatus here described, which has the advantage of cheapness, consists of a revolving drum on the surface of which are made contact and break surfaces. A thermograph is readily adapted to this purpose, as illustrated in figure 1. The thermograph is insulated

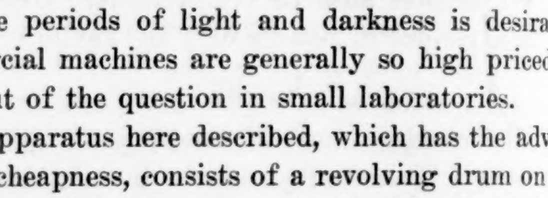

Fig. 1.

at *A* by a cone of fiber paper, and at the point *D* by fiber board. The lower end and the outer wall of the drum are brightened to make contact with *B* and *C*. Then a band of fiber paper *F* is held in place around the drum by two rubber bands *R*. Seven triangular pieces are cut from this band of fiber paper as shown in figure 1, to allow the point *B* to make contact with the drum. When this point comes in contact with the drum, the magnetic switch, No. 2829653Z2 General Electric, *M* closes the power circuit *P*, and the lights are on. As the point *B* runs onto the fiber paper breaking the control circuit the magnet is demagnetized, and the lights are turned off.

By simply raising or lowering the point *B*, by means of the adjustable support *E*, a long or short illumination period may be obtained. The drum is adjusted to the particular time of day requiring illumination, and the clock wound once each week.

Fig. 2.

If illumination is required for any length of time during two or more different periods of the day, the band of fiber paper is cut accordingly. Figure 2 represents the type of band for any length period of morning and evening illumination.

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CENTRIFUGING FILTERABLE VIRUSES

FROM time to time there have appeared experimental reports in which attempts to concentrate filterable viruses by the centrifugal method have been described. Particles of such small size are incapable of any great velocity of sedimentation even when acted upon by centrifugal force. In a general sense, the same is true of bacteria.

By Stoke's law; the velocity $\mathbf{v}$ of a sphere of radius r and of density Δ falling under gravitational acceleration g in a liquid of density δ and viscosity η is:

$$\mathbf{v} = \frac{2r^2 g(\Delta - \delta)}{9\eta}$$

Substituting for g, gravitational acceleration, the centrifugal acceleration—

$$\omega^2 R = 4\pi^2 R P^2$$

where ω is angular acceleration, R is the radius of curvature (from the center of the centrifuge to the particle), and P is the angular velocity, or revolutions per second, we have:

$$\mathbf{v} = \frac{2r^2(\Delta - \delta)}{9\eta}(4\pi^2 R P^2) = \frac{8\pi^2 r^2 R P^2(\Delta - \delta)}{9\eta}$$

This, then, is the general equation.

Let us now solve for $\mathbf{v}$ in a general problem. We assume a virus particle 5×10^{-6} cm. in radius ($0.1\,\mu$ diameter), spherical, of density 1.1.[1] Let it be suspended in a liquid of density 1.0 and located 20 cm. from the center of the centrifuge. Let the viscosity be 0.01 (water at 20° C.) and the speed be 3,600 r.p.m. (P = 60). Then:

$$\mathbf{v} = \frac{8\pi^2(5 \times 10^{-6})^2 \times 20 \times 60^2(1.1 - 1.0)}{9 \times 0.01}$$
$$= 158 \times 10^{-6} \text{ cm./sec. or } 0.57 \text{ cm./hr.}$$

This velocity is certainly not great, since under the conditions stated some 8.8 hours of centrifuging would be necessary to carry a particle 5 cm. And if analysis is made of the values used in this problem it will be seen that they are taken to give $\mathbf{v}$ a probable maximum value. The viscosity in practice is ordinarily greater than that of water, and the radius of the particle is almost unquestionably less than 5×10^{-6} cm. Ordinarily centrifuge methods applied to filterable viruses are from the standpoint of physical laws of questionable value.

The surface-volume relationship in the illustration problem is such that a 1 cc. volume would have to be contained in a film less than a micron thick and over half a meter square to give relatively the surface exposure, considering both sides of the film. Or a centimeter cube with its 6 cm.² surface would have to have a density about 1/100 that of air to give the same surface-mass relationship as pertains to the minute particle described.

Thanks are due to Mr. W. W. Sleator of the laboratory of physics of the University of Michigan for checking and correcting this problem.

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[1] Investigation of a number of references on the density of bacteria gives various figures. A density of 1.1 is considered a fair average.

PERSIMMON SEEDS FOR CLASS USE

AN examination of the seeds of the common persimmon, *Diospyros virginiana*, convinced the writer that they should make excellent class material for embryological studies as well as for studies of the structures of a thick-walled endosperm. The comparatively large, straight embryo is easily removed from the endosperm and its parts are easily seen. Younger stages should make good microscopic preparations for embryological work, provided that the difficulties encountered in cutting the testa and endosperm are not too great. Carbohydrate is apparently stored in the thick cell walls of the endosperm in the form of cellulose or hemi-cellulose, and this being the case, the germinating seeds should be a good source of cytase-like enzymes.

During the past season the writer sent a supply of persimmon seeds to Dr. E. M. Gilbert, of the department of botany of the University of Wisconsin, who writes that they have been used successfully in several classes for embryological work and for studies of the thick-walled endosperm. Dr. Gilbert further states that the walls of the endosperm have proved to be unusually good material for the study of the plasmodesmus.

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SPECIAL ARTICLES

THE OCCURRENCE OF THE PLATINUM METALS

RECENTLY I have had the opportunity of studying the results of some forty analyses of the Canyon Diablo meteorite, both of the iron and of the so-called shale-balls. The latter appear to be merely the oxidized iron, as some of them still have an unoxidized iron core. The analyses were made by at least eight different analysts, including Dr. J. W. Mallet, H. H. Alexander, A. H. Phillips, G. H. Clevenger and myself, and included the content in platinum, iridium and in some cases palladium.

There have been but two references in literature to the occurrence of platinum in meteorites.[1] Trottarelli reported palladium in the Collescipoli stone, and J. M. Davidson found the Coahuila iron to contain 39 parts per million of platinum and 2.44 parts iridium. In the Toluca meteorite sufficient platinum was found to give a precipitate of potassium chloroplatinate, which from its color probably contained iridium. No quantitative estimation was made.

The Canyon Diablo analyses, weighted according to my best judgment, average as follows:

Platinum	11.2 parts per million
Iridium	5.8 parts per million
Palladium	2.1 parts per million

The ratio of platinum to iron in the shale-balls corresponds closely to that in the unoxidized meteorite, the ratio of iridium to iron is lower and that of palladium to iron somewhat higher.

The average amount of nickel found in all the analyses, not weighted, is 6.44 per cent. Clarke gives the average nickel for 318 meteorites as 8.52 per cent., and in the Ovifak iron 2.95 per cent.

It may be considered probable that platinum, and doubtless all the platinum metals, would be found in all meteorites if analyses were made with this end in view, though the estimation of three or four tenths of an ounce of platinum and iridium to the ton of meteoric iron is no simple task. It may be noted that in dissolving the iron in either sulfuric or in hydrochloric acid, some of the platinum and iridium will go into solution, and this doubtless accounts for the varying results on the Canyon Diablo iron where such a method has been used.

Attention has been called by many observers to the association of the metals of the eighth group in nature. In 1891 Daubrée and Meunier noted the occurrence of metallic iron containing traces of platinum in the gold washings of Berazovsk in the Ural, and also that many meteorites resembled rocks with which platinum is generally associated in nature.

It may be worth while to attempt a rough approximation of the relative amount of the metals of the eighth group, assuming that the iron of the interior of the earth contains the same proportion of the platinum metals as the Canyon Diablo meteorite.

For this we can use the calculation of F. W. Clarke for the earth as a whole:

Iron	67.2 per cent.
Nickel	4.0 per cent.
Cobalt	.277 per cent.

Average cobalt in 318 meteorites: 0.59 per cent.

Clarke gives the analyses of 8 native platinums and 3 iridosmiums, and Kemp gives 42 analyses of native platinum and 12 of iridosmium. From these we derive the following weighted averages:

	Native platinum	Iridosmium
Platinum	89.88	.48
Iridium	4.88	60.37
Osmium	1.92	33.53
Rhodium	2.47	3.59
Palladium	.83	Trace
Ruthenium	.011	2.05

Recent figures[2] give the composition of Russian crude platinum as: platinum, 83 per cent.; iridium, 2 per cent.; palladium, 0.5 per cent.; rhodium, 0.6 per cent.; iron, etc., 13.9 per cent. It is doubtful if these figures can be relied on as general.

An approximation of the amount of iridosmium compared with platinum can be made from the amount produced over a long period of years. The present proportion (1925) of 9 per cent. as much iridosmium as platinum is obviously too large, owing to the stimulation of production by the abnormally high price of iridium, while the earlier production of 1 per cent. to 3 per cent. is as obviously low, from the slight market demand for iridosmium and the metals obtained from it. We may fairly assume 5 per cent. as about the proper proportion of iridosmium to platinum. On this basis, our figures for

[1] Trottarelli: *Gazz. chim. ital.* 20 (1890), 611; Davidson: *Amer. J. Sci.* (4), 7 (1899), 4.

[2] *Afr. Mining Eng. J.* 38 (1927), 123.

the relative amounts of the platinum metals in nature, considering platinum as 100,000, become:

Platinum	100,000
Iridium	8,793
Osmium	4,046
Rhodium	3,947
Palladium	924
Ruthenium	236

Using the estimate of these metals in the Canyon Diablo meteorite, and combining it with Clarke's estimate of the relative amounts of iron, nickel and cobalt, we arrive at the following figures for the amount of the metals of the eighth group in the earth, considering iron as 1,000,000,000.

Calculating from *platinum* in the Canyon Diablo meteorite:

Iron	1,000,000,000
Nickel	59,524,000
Cobalt	4,122,000
Platinum	12,043
Iridium	1,055.1
Osmium	488.5
Rhodium	343.6
Palladium	106.8
Ruthenium	28.3

If the calculations are based on the *iridium* reported in the Canyon Diablo meteorite, the figures become:

Iron	1,000,000,000
Nickel	59,524,000
Cobalt	4,122,000
Platinum	70,926
Iridium	6,236.6
Osmium	2,868.0
Rhodium	2,094.7
Palladium	656.8
Ruthenium	167.9

but owing to the difficulty of determining iridium accurately, it is doubtful if these figures can be considered reliable.

If calculation were made from South African iridosmium, the osmium figures would be larger, as this iridosmium apparently runs much above the average in osmium; on the other hand, osmium analyses are apt to be low, owing to volatilization. The palladium is lower than would be anticipated; in the Sudbury ores the palladium runs much higher in proportion to the platinum. The ruthenium is unexpectedly low, but is probably approximately correct.

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THE ENCYSTMENT OF PARAMOECIUM IN THE RECTA OF FROGS

So far as I know *Paramoecium* has not been definitely shown to encyst in nature nor in laboratory cultures. In fact, most of the investigators who have worked with this organism state that they have never seen it encyst and are of the opinion that it does not possess the ability to do so. Hence the following observations, though incomplete, seem worthy of record.

Two to three c.c. of rich, milky-white cultures of *Paramoecium* (species not determined) were injected (by attaching a short catheter to a syringe) into the recta of frogs, with the result that encystment occurred in about two per cent. of the frogs injected. In all, encystment has been observed in three frogs. When it was first observed, two hours had elapsed since the paramoecia were introduced into the rectum. When a portion of the rectal contents was examined a fair number of individuals were observed in what later seemed to be the beginning of encystment, although at first they were very nearly overlooked for *Opalina*. More careful observations, however, disclosed a very thin membrane-like substance surrounding them. By continued observations it was finally possible to observe six individuals regain their normal *Paramoecium* shape, appearance and activity by freeing themselves of the peculiar substance enclosing them. It was really not possible until the organisms had freed themselves of the membranes to determine whether I was observing *Paramoecium*, some undescribed parasitic ciliate of frogs, or *Opalina* in an abnormal condition, because they presented a very unusual appearance due to the fact that they were folded and rounded so as to occupy about half their normal space. Others, however, were not able to free themselves and, after an hour or two, became more and more rounded and definitely enclosed within what, by this time, could be called a definite membrane—perhaps a cyst membrane.

In another frog which was examined five and a half hours after injection per rectum only encysted paramoecia were present. These were placed in three depression slides, four organisms on one slide and several on each of the others, and kept in a moist chamber and observed several times daily. No change was noticed for the first three days, but on the fourth day some of the cysts were undergoing fission, and on the fifth day two organisms were seen within a single cyst. A fairly heavy cyst wall was clearly visible. On the fifth day some paramoecia excysted.

When a considerable amount of tap-water was added, it was noticed that very soon the movement, which had been quite slow, was gradually increased and in three instances was observed to bring about excystation after two hours. As the movement of the organism became more rapid the cyst wall became thinner and thinner until the organism was finally able to free itself and swim away. Shortly before the organism was free, it could be seen pushing against the cyst wall which by this time had become a very thin membrane which would bulge out as the organism pushed against it from within. Several of the cysts produced in this experiment were observed for eight days when they were accidently lost due to evaporation of the water containing them. None of the encysted paramoecia were ever observed to lose movement entirely, although movement in some was very feeble indeed.

In another instance paramoecia were injected into the recta of five frogs. After four and a half hours the frogs were examined; four contained a few free and fairly active paramoecia and no cysts, and one contained cysts with thick heavy walls and no free paramoecia.

In many instances the paramoecia were all dead within three to four hours after injection into the frog's rectum. A very high percentage were killed and disintegrated (digested perhaps) within one to two hours, or before encystment was ever observed to occur.

All attempts to bring about encystment in removed recta, in removed rectal contents, in the recta of killed frogs, and in the stomach and intestines failed.

We may have in these meager observations an inkling as to the origin of parasitism; during the protection afforded by encystment a free-living organism may gradually become acclimatized or adapted to its new and unfavorable environment and finally become a parasite. It would perhaps be a worth while undertaking to place in the alimentary tract and in the tissues of animals the cysts and free forms of some of the well-known free-living ciliates which form cysts readily in nature and in culture. After thousands of failures it might be possible to find an organism that could excyst and then maintain itself on the intestinal bacterial flora.

L. R. Cleveland

NATURAL AND EXPERIMENTAL INGESTION OF PARAMOECIUM BY COCKROACHES

About thirty cockroaches were collected in the basement of a department store in Baltimore between eight and nine in the morning. They were placed in a dry bottle and carried to the laboratory where several were dissected about two hours later and their stomach and rectal contents examined microscopically. Three of those examined had *Paramoecium* in their stomachs. No attempt was made to determine the species of *Paramoecium,* but the observation was verified by three individuals in the laboratory who were familiar with this well-known organism. The usual parasitic protozoa were seen in the rectal contents, but *Paramoecium* was only observed in the stomach. The remaining cockroaches (17 in all) were left to be examined later to see if *Paramoecium* remained present and if it reached the rectum. These were all opened up and observed between three and four in the afternoon, seven to eight hours after they were collected, and no living paramoecia were present in any of them, but the remains of paramoecia were clearly visible in the stomach contents of two individuals.

In order to determine how long *Paramoecium* would live in cockroaches, approximately two hundred individuals were collected and were starved in dry petri dishes from one to ten days before being fed a rich, milky-white culture of *Paramoecium,* containing hundreds of individuals per drop. In most of the experiments the cockroaches were starved four or five days because it was somewhat difficult to get them to ingest paramoecia after one to two days' starvation. Each cockroach was placed in a petri dish and was observed until it ingested from one to three drops of the culture, the time was noted, and then the cockroach was removed from the petri dish with forceps, swabbed off with cotton, and placed in another petri dish with blotting paper in the bottom. A hundred and fifteen observations were carried out in this manner. The cockroaches were dissected at intervals from five minutes to three days after having been observed to feed on *Paramoecium.* The contents of their alimentary tracts were examined microscopically, with the result that few, if any, of the paramoecia were killed during the first two hours after ingestion and that all were killed by the end of five hours except in a single instance where three actively motile paramoecia were found in the stomach contents six hours after ingestion. When the stomach contents were examined three and four hours after the ingestion of paramoecia, mostly broken up or disintegrating organisms were observed together with three or four normally active individuals. This, then, makes it highly probable that the cockroaches in which *Paramoecium* naturally occurred had fed on water containing a fair number of these organisms shortly before they were collected and brought to the laboratory—perhaps a rather unusual occurrence.

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